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Since the early 1960s, the federal government has been involved in the direct creation of new employment opportunities, especially at the state and local level. Commerce Department programs provide intergovernmental grants, business loans, planning assistance and funds for public works ("infrastructure"), all aimed toward stimulating private investment in new (or retooled) productive capacity. The Community Services Administration (formerly OEO) allocates equity capital to community development corporations to facilitate the creation or retention of locally-owned and managed "quasi-public" enterprises in poverty areas. And since 1971, the Department of Labor has operated a series of large (and growing) programs to finance public service employment in state and local governments. Other, analogous programs are currently on the drawing boards of

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Washington planners, and now a number of state governments have begun to undertake job-creation experiments of their own.[1/] Given the prevailing forecasts of continued high unemployment for the American economy -- especially for its older urban and rural areas -- there can be no doubt that these efforts to supplement aggregate fiscal and monetary policy with programs to "target" job-creation to disadvantaged workers and places will grow in the years to come.

In the evaluation of any such program, one especially thorny methodological problem presents itself. How are we to decide what would have happened to the workers who actually get the newly-created jobs, in the absence of the program? Suppose (say) a public works or public service employment program provides a budget for 100 weeks of full-time wages, salaries and overhead. Does this mean that 100 weeks-worth of new work has been created? Probably not. From the point of view of both the individual enrolled in the position and the program managers, the net change in employment (expressed in weeks) is equal to the difference between 100 and the number of weeks that the enrollee could have expected to work if the job-creation program had never existed. Since most workers face the prospect of at least some employment over the course of a year,[2/] the net increment to their work experience probably is less than 100 (weeks).

1/ Cf. Audrey Prager, et al., Job-Creation in the Community: The Massachusetts Local Initiatives Program (Cambridge: Abt Associates, 1977).

2/ In 1975, only 15% of those with some unemployment during the year were without work the entire time. US Dept. of Labor, Employment and Training Report of the President (Washington, DC: Government Printing Office, 1977), p. 216.

We could, of course, simply assume either that the program creates no additional employment for the individuals involved (everyone enrolled would have worked for the same total number of weeks over the program period) or that all the employment is additional (none of the participants would have found any work without the program). This latter assumption might conceivably hold for those who were either structurally unemployed or who were brought back into the labor force as a result of the program. The former might hold true for participants who were either employed prior to the program or were unemployed for a very short (frictional) time before their entry into the program. The truth, of course, lies somewhere between the extremes.

The ideal solution to this "unobserved variable" problem in program evaluation is to draw an independent control group of non-program participants at each project site, matched to a sample of program enrollees by age, race, sex, prior labor market experience, etc. and then to compare their experiences with participants during the program period. Unfortunately, as is well known to program evaluators, control groups are expensive to find, feed and follow. And even the absolute technical superiority of this method has been called into question by some evaluators.[3/]

Our solution to the problem has been to make use of recent developments in the quantitative modeling of the process by which workers move in and out

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[3/] Cf. "Three Perspectives on Social Science Research", Monthly Labor Review, Feb. 1972, on cross-section sample surveys, longitudinal surveys, and experiments with control groups.

of the various "states" in the labor market.[4/] Using an econometric model of employment transitions for various age-race-sex cohorts of the national population, we simulate the expected work experiences of each cohort over time. From these simulations, we are able to extract estimates of the expected amount of time at work for each cohort. These statistically simulated weeks of work are then used to discount the actual volume of work generated by the Job Opportunities Program of the Economic Development Administration, a countercyclical job-creation program that was in effect between June 1975 and July 1977. The "Title X" Program (so-called because it was passed into law in 1975 as a new Title X to the Public Works and Economic Development Act of 1965) directly funded over 75,000 person-years of work and is quite typical of the kind of short-term targeted job-creation program which we are likely to see more of in the years ahead.

Project Evaluation in a Federal System: The Interdependence of Efficiency and Equity Considerations

Before proceeding to a description of the model and how it was used to assist us in evaluating the EDA's Title X program, we want to place this particular methodological problem into a larger context. There has been

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[4/] There has been much research into these issues, especially in reference to national employment policy. See, for example, George L. Perry, "Unemployment Flows in the US Labor Market", Brookings Papers on Economic Activity, Vol. 2, 1972; Stephen Marston, "Employment Instability and High Unemployment Rates", BPEA, Vol. 1, 1976; Robert L. Crosslin and David W. Stevens, "The Asking Wage-Duration of Unemployment Relation Revisited", Southern Economic Journal, Vol. 43, No. 3, January 1977; Charles Holt, "Modeling a Segmented Labor Market", in Phyllis A. Wallace and Annette M. La Mond, eds., Women, Minorities, and Employment Discrimination (Lexington, Mass: D.C. Heath, 1977).

growing discussion of late about the many ways in which local implementation of an initially federally promulgated program can undermine the intentions of the federal planners. We are bombarded by a whole new set of technical terms that have something to do with these intergovernmental relations: "budget substitution effects", "displacement effects", "vacuum effects". Our objective in this brief section is to sort out these concepts and to show how their interpretation relates to questions of who benefits (and loses) from job-creation policy as well as the question of how many jobs are "really" created. This will give us a structure within which to pose our own particular question: how much of the labor demand created by a government program would have been available to the enrollees anyway, even if the program had never existed?

Consider the federally-funded local employment project designed to create 100 person-weeks of new labor demand (fig. 1). Let us assume that the local government hires new enrollees to perform 50 weeks of work. The other 50 weeks of labor are performed by workers already "on board" in the local government. This is the budget substitution effect; in fig. 1, it occurs at a rate of 50/100 or .5. Moreover, in the absence of the federal program, suppose that the new enrollees could have found 10 weeks of work anyway, somewhere in the economy, so that only 40 weeks of new work ("new" to the enrollees) is actually created. Now, suppose in addition that the local government deliberately chooses not to use 20 of the weeks for which it has funding, and implements this decision by displacing (i.e. laying off) people who would have performed this work to make room for newly hired enrollees in the program. This is a displacement effect of 20/50 = .4.

But then only 30 of the 50 "new" person-weeks of work are truly new. Moreover, only 40/50 of those weeks, or 24 weeks, provide work that the new job-holders wouldn't have had in the absence of the program. Note that the substitution and displacement processes free up 70 person-weeks worth of resources which the local jurisdiction can use for a variety of other purposes, from direct expenditure to debt service or even tax relief.

It seems to us that there is no one employment impact to be read from this scenario. The results depend crucially on the viewpoint. Suppose we evaluate the outcome from the perspective of the program managers. The federal objective was to create 100 weeks of project employment. The local government is actually using only 80 weeks, having chosen (via displacement) not to use 20 person-weeks worth of resources. But, as we have just seen, not all of these 80 weeks constitute new work.

$$\text{net impact to program managers} = \left[\left(\text{intended impact}\right)\left[1 - \left(\text{rate of budget substitution}\right)\right]\left(1 - \left[\frac{\text{weeks of work expected by employees in absence of program}}{\text{weeks of work funded by program (i.e. intended impact)}}\right]\right)\right] - \text{weeks of displaced employment} + \text{weeks of work expected by the displaced employees}$$

Assume that, just as new employees could have found a fifth of their new weeks of work elsewhere in the absence of the program, so a fifth of the displaced person-weeks can be made up elsewhere in the economy; 1/5 of 20 = 4.

Applying this algorithm to the example in figure 1, net impact is $\left[(100)\left(1 - \frac{50}{100}\right)\left(1 - \frac{10}{50}\right)\right] - 20 + 4 = 24$ new weeks of job-creation.

A second perspective for program evaluation is that of the new employees themselves. Since displacement does not directly affect them,

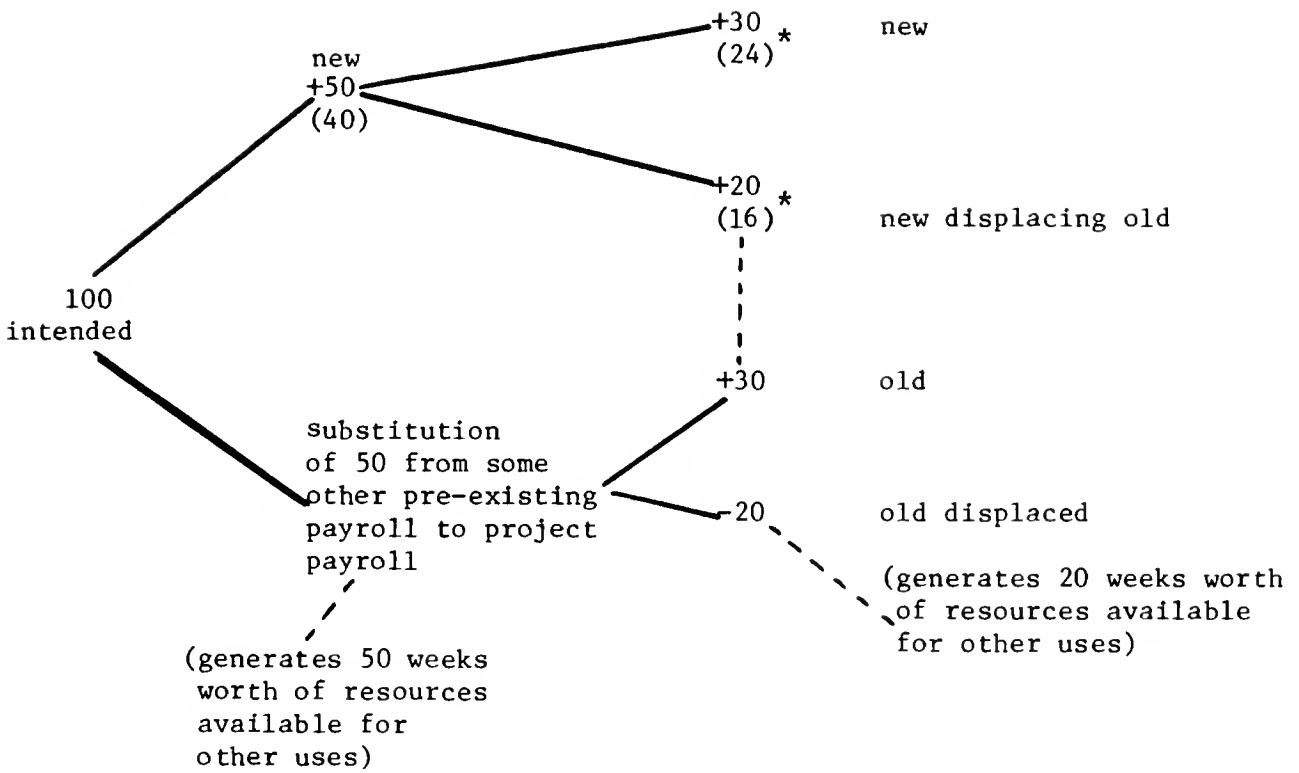

Figure 1

Possible Allocation of Federal Resources
Within a Local Project

*obtained by applying the proportion 40/50
to each of the components 30 and 20.

$$\text{net impact on new employees} = \left(\text{intended impact}\right)\left[1 - \left(\text{rate of budget substitution}\right)\right]\left(1 - \left[\frac{\text{weeks of work expected by enrollees in absence of program}}{\text{weeks of work funded by program (i.e. intended impact)}}\right]\right)$$

Using the numbers in our example, new employees gained

$$\left[(100)\left(1 - \frac{50}{100}\right)\left(1 - \frac{10}{50}\right)\right] = 40 \text{ new weeks of work.}$$

There is yet a third viewpoint for evaluating the outcome: that of the economy as a whole (the macroeconomic or "social" perspective). The federal government provided 100 person-weeks worth of new resources. The local government directly used only 80 weeks worth. Moreover, 50 of those 80 weeks of labor demand were filled by persons already on board, reducing the new demand to 30 weeks. From the social perspective, the full 30 is relevant, rather than the 24. If we assume that there is still a significant amount of involuntary unemployment in the economy as a whole, even the fact that project employees could have found 6 weeks worth of work in the absence of the program is no longer relevant; those 6 weeks will be claimed (or the slots will become occupied) by people who are now presently unemployed somewhere: the vacuum effect. Moreover, the social (as opposed to either the managerial or private) impact is much greater than 30, because 70 weeks worth of job-creating resources have been freed up for use outside the program. As Charles Killingsworth has pointed out, there is no reason not to assume that these resources will not ultimately be respent somewhere in the economy.[5/] To the extent that they are respent (rather than saved), and that the new uses generate an equivalent demand for labor, the net social impact will approach the intended impact of 100 new person-weeks of labor demand.

5/ Charles C. Killingsworth, "The Role of Public Service Employment", Industrial Relations Research Association/Proceedings, 1977 Annual Spring Meeting, 1977.

So much for the theoretical possibilities. Do we have any empirical evidence on these parameters of sub-federal fiscal behavior? Some econometric research has been conducted on the substitution effect, indicating rates of anywhere from 40% to 90% in the short run.[6/] But Killingsworth quite rightly says of these estimates that

> the studies on which (they) relied were based on speculation to such an extent that they could not properly be given any weight as evidence...far too much has been made of the "substitution" argument. We have no solid evidence concerning its extent; in fact, no one has even thought of a reliable way to measure it.[7/]

Wiseman also calls the prevailing estimates "seriously flawed".[8/] As far as the evidence of the displacement effect is concerned, we have only anecdotal accounts so far.[9/] It seems to us that, for rather obvious political reasons (displaced persons will often get to vote their dissatisfaction later), this effect is likely to be small.[10/]

Thus we can see that some measure of a program enrollee's expected work

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6/ National Planning Association, An Evaluation of the Economic Impact of the Public Employment Program (Washington, D.C.: N.P.A., 1974); George Johnson and James Tomola, "The Efficacy of Public Service Employment Programs", US Dept. of Labor, Office of Policy Evaluation and Research, Technical Analysis Paper 17A, 1975. These and other related studies are critically reviewed by Michael Wiseman, "Public Employment as Fiscal Policy", Brookings Papers on Economic Activity, Vol. 1, 1976.

7/ Killingsworth, op.cit.

8/ Wiseman, op.cit., who also offers the observation that the problem itself seems correctable by appropriate administrative sanctions or some sort of performance contracting system.

9/ "Chicago to Repay Title VI Funds Misused in City's Jobs Program", ETA Interchange, US Dept. of Labor, October 1977, p.2.

10/ For completeness, we may mention another factor which has to be considered by program evaluators. The creation of jobs or -- more often -- training slots intended for some target group may attract new entrants into the labor force, some of whom may actually get the new jobs. Whether or not this affects the private or social benefits of the program in terms of actual new work created, it will prevent the unemployment rate from declining as much as would have occured in the presence of an inelastic supply of labor. Thus, the value of the official unemployment rate as an indicator of program impact is compromised when labor supply is variable.

experience in the absence of that program is an important parameter in evaluating the net benefits to the enrollee him(or her)self, as well as to those officials in charge of the program (it is not, however, germane to the estimation of net social benefits). We now proceed to an explanation of how we estimated that parameter in connection with our evaluation of the Job Opportunities Program of EDA.

The Labor Market Transition Model

In the course of our research, we became aware of the job-search-theory-based work at the Urban Institute, specifically the "Inflation and Unemployment Project" directed by Dr. Charles Holt. With the enthusiastic support and advice of Drs. Holt, Ralph Smith and Jean Vanski, we proceeded to adapt the model constructed at the Institute to meet our own program evaluation needs.

Smith, Vanski and Holt had collected month-to-month "gross flows" data from the Current Population Survey, covering the period between July 1967 and December 1973 (5.3 years x 12 months/year = 66 months of data), a period including one complete business cycle. For each of sixteen demographic groups:

male, white, aged 16-19
" 20-24
" 25-59
" 60+

male, nonwhite, aged 16-19
" 20-24
" 25-59
" 60+

female, white, aged 16-19
" 20-24
" 25-59
" 60+

female, nonwhite, aged 16-19
" 20-24
" 25-59
" 60+

Smith, et al., constructed a string of month-to-month employment status transition matrices, with the following structure:

	Employed this month	Unemployed this month	Not in the labor force this month
Employed last month	p_{11}	p_{12}	p_{13}
Unemployed last month	p_{21}	p_{22}	p_{23}
Not in the labor force last month	p_{31}	p_{32}	p_{33}

Each element (p_{ij}) represents the probability that a person of the corresponding age/race/sex will find him(her)self in a particular status this month, given his(her) status last month. The probabilities sum to 1.0 (or a hundred percent) along the rows. Thus, for example, of everyone employed last month, p_{11} were employed this month, p_{12} were unemployed, and p_{13} dropped out of the labor force; $p_{11} + p_{12} + p_{13} = 1.0$. For each demographic group, we therefore have a string of (66-1=)65 transition matrices which describe how people of given age/race/sex move in and out of jobs and in and out of the labor force from month to month.

Smith et al. then employ multivariate regression analysis to measure the correlates of these month-to-month variations in the p's.[11/] Simplifying

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11/ Ralph E. Smith, "A Simulation Model of the Demographic Composition of Employment, Unemployment, and Labor Force Participation", in Ronald G. Ehrenberg, ed., Research in Labor Economics, Vol. I (Greenwich, Conn: JAI Press, 1977).

their presentation somewhat, we have the equivalent of (9x16=)144 equations, one for each type of transition for each demographic group:

p_{dijt} = f (time trend; seasonal adjustment factors, ratio of the Conference Board's estimated level of job vacancies to the Current Population Survey unemployment level lagged one month, called V/U)

where d = 1,2,...,16 demographic groups

i = 1,2,3 initial "states" (last month's status)

j = 1,2,3 current states (this month's status)

and there are t = 1,2,...,65 month-to-month changes being observed. The ratio (V/U) in month (t-1) is an important variable measuring the tightness of the labor market, i.e. "the availability of jobs in relation to the availability of people to fill them".[12/]

From actual program data, we know the age, race, sex, program entry and exit months, and month of enrollment of each Title X Job Opportunities Program enrollee. From CPS and Conference Board data, we can measure $(V/U)_{t-1}$, the labor market conditions in each of the enrollee's months in the program (lagged one month). By substituting the appropriate Title X project period data into the Urban Institute equations, we can forecast the expected pattern (time path) of transition probabilities for each Title X employee. Appropriate cumulation of the forecasted conditional probabili-

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12/ Ibid. Smith notes that "The US does not have comprehensive vacancy statistics. We assume an average vacancy level during the 1967-73 estimation period of about 2 million and use the Conference Board's Index to measure the variation (from month to month) in the level."

ties of being employed each month,[13/] counted over the range of months in the program, then yields the needed estimate of the unobserved variable: the number of weeks that the enrollee would have worked in the absence of EDA's Job Opportunities Program.

First, however, we must realize that the Smith et al. model is not quite so straightforward as we have presented it above. In particular, it is aggregated into cohorts (i.e., it does not literally forecast the mobility patterns of individuals), and the population of each cohort varies from month to month, changing the size of the pool of people entering the system being modeled (in other words, the system is not closed). Operationally, this means that the transition process is not strictly Markovian, so that nine transition probabilities p_{ij} cannot in fact be consistently estimated by nine equations.

The Smith et al. solution is to estimate the nine transition probabilities (for each of the sixteen demographic groups) with six equations and five identities that allow for an intermediate state called "labor force (re)entry" (which may or may not be successful in terms of whether or not it results in finding a job) and which account for month-to-month variations in the size of the population in the system. The six behavioral equations which -- together with two identities -- permit estimation of the off-diagonal probabilities are of the form:

13/ The algorithm for this cumulation, reproduced below, assumes that transition from one month to the next is a first-order Markovian process, i.e. that where you end up next month depends only on where you were the month before, and not on how you got to that point. This is a simplification of the transition process, no doubt, but it is the only assumption consistent with the first-order difference equation form of the Urban Institute model.

$$P_{ijt} = \alpha \left(\frac{V}{U}\right)^{\beta}_{t-1} e^{\gamma T} e^{\sum_{t=1}^{11} \delta_t S_t}$$

e.g., for the unemployment-to-employment transition,

$$\ln_e \left(\frac{UE_t}{U_{t-1}}\right) = \ln_e \alpha + \beta \ln_e \left(\frac{V}{U}\right)_{t-1} + \gamma T + \sum_{t=1}^{11} \delta_t S_t$$

where the S's are seasonal dummies, indexing the month, and T is an annual time trend indexing the year. There are three identities for the diagonal probabilities, constructed to allow for changes in the various stocks in the model (population, total labor force, etc.).

Smith et al. run their estimated model by inputting initial stock values, simulating one month's flows, and then observing the new stock values. These endogenously generated stocks then become input into the next month's simulation and so on. Since we have exogenous vacancy and unemployment data for each month of our simulation period (1975-77), from Conference Board and CPS records, we chose to use the Urban Institute model to compute wholly self-contained month-by-month forecasts. That is, instead of simulating new stocks each month, we entered them anew, from outside the model. The mechanics were simplified in this way, but our forecasts became inconsistent, occasionally producing transition matrices whose row sums exceeded unity. To adjust for this, we decided to use only the six behavioral equations and two identities that generate the off-diagonal probabilities, and to constrain the diagonal probabilities to values that would preserve the condition that row elements sum to unity. There is some loss of information here, because the six behavioral equations do not take into account the levels of stocks of

employed persons, unemployed persons, etc. But since our subsequent simulation of cumulative weeks worked (see below) requires that the strings of forecasted transition matrices be consistent with a first-order Markovian transition process (i.e., row elements sum to unity), we really had no choice.[14/] The matrices for a sample of cohorts are given in Tables 1-4.[15/] Inspection reveals realistic patterns, especially in terms of seasonality (e.g., young workers show the most volatile changes in labor force entry and exit at the beginning and end of summer). It remains now to explain how we use these forecasted transition tables to estimate "weeks worked in the absence of Title X".

14/ It is hard to pin down the cause of our inability to produce consistent forecasts with the Urban Institute model in its original form, because the behavioral equations in that model were estimated by Ordinary Least Squares. Since the dependent variables are probabilities bounded by 0 and 1, OLS produces estimates which are heteroscedastic, and which do not rule out forecasts that fall outisde the 0-1 interval. The correct estimation technique is probit analysis (since those left-hand variables, at the Smith et al. level of aggregation, are ratios). We considered re-estimating the basic equations themselves, but the cost would have been prohibitive. The upshot is that forecasts of transition probabilities that do not sum to unity across rows are quite possible with the system that the Urban Institute has generated.

15/ The complete set of 16 strings of transition matrices is available in Abt Associates, Effects of Job Creation, Volume Four, Appendix B, Cambridge, 1977. The format of these tables corresponds to the matrix described on p. 11 above.

Table 1

EXPECTED TRANSITION PROBABILITIES OF NONWHITE WOMEN AGED 20-24

NOV 1975

0.96	0.01	0.03
0.03	0.70	0.26
0.03	0.07	0.90

DEC 1975

0.96	0.01	0.03
0.04	0.69	0.27
0.03	0.07	0.90

JAN 1976

0.95	0.01	0.04
0.05	0.62	0.34
0.03	0.07	0.89

FEB 1976

0.97	0.01	0.02
0.02	0.71	0.27
0.02	0.07	0.91

MAR 1976

0.97	0.01	0.02
0.03	0.68	0.30
0.02	0.07	0.91

APR 1976

0.97	0.00	0.03
0.03	0.73	0.24
0.00	0.10	0.90

MAY 1975

0.97	0.01	0.03
0.04	0.69	0.27
0.03	0.07	0.91

JUN 1976

0.96	0.01	0.03
0.04	0.67	0.29
0.03	0.10	0.86

JUL 1976

0.96	0.01	0.03
0.03	0.70	0.27
0.04	0.09	0.87

AUG 1976

0.96	0.01	0.03
0.04	0.72	0.25
0.04	0.07	0.89

SEP 1976

0.95	0.01	0.04
0.05	0.71	0.24
0.04	0.09	0.88

OCT 1976

0.97	0.01	0.03
0.04	0.72	0.24
0.03	0.08	0.89

NOV 1976

0.96	0.01	0.03
0.04	0.70	0.26
0.03	0.08	0.89

DEC 1976

0.96	0.01	0.03
0.04	0.69	0.27
0.03	0.07	0.90

JAN 1977

0.95	0.01	0.04
0.05	0.62	0.34
0.03	0.07	0.89

FEB 1977

0.97	0.01	0.02
0.02	0.71	0.27
0.02	0.07	0.91

MAR 1977

0.97	0.01	0.02
0.03	0.68	0.30
0.02	0.07	0.91

APR 1977

0.97	0.00	0.03
0.03	0.73	0.24
0.00	0.10	0.90

MAY 1977

0.97	0.01	0.03
0.04	0.69	0.27
0.03	0.07	0.91

JUN 1977

0.96	0.01	0.03
0.04	0.67	0.29
0.03	0.10	0.86

JUL 1977

0.96	0.01	0.03
0.03	0.70	0.27
0.04	0.09	0.87

Table 2

EXPECTED TRANSITION PROBABILITIES OF WHITE WOMEN AGED 20-24

NOV 1975			DEC 1975			JAN 1976			FEB 1976		
0.94	0.01	0.05	0.93	0.01	0.06	0.92	0.02	0.06	0.94	0.01	0.05
0.29	0.57	0.14	0.26	0.60	0.14	0.23	0.61	0.16	0.25	0.61	0.14
0.06	0.06	0.88	0.06	0.06	0.88	0.05	0.06	0.90	0.06	0.07	0.88
MAR 1976			APR 1976			MAY 1976			JUN 1976		
0.95	0.01	0.05	0.94	0.01	0.05	0.94	0.01	0.05	0.92	0.01	0.06
0.22	0.63	0.15	0.24	0.61	0.16	0.24	0.61	0.15	0.26	0.61	0.13
0.05	0.06	0.89	0.05	0.06	0.89	0.05	0.05	0.89	0.10	0.10	0.81
JUL 1976			AUG 1976			SEP 1976			OCT 1976		
0.93	0.01	0.06	0.93	0.01	0.06	0.89	0.01	0.09	0.94	0.01	0.05
0.31	0.56	0.13	0.28	0.59	0.13	0.28	0.59	0.13	0.30	0.58	0.12
0.07	0.07	0.86	0.06	0.07	0.86	0.08	0.08	0.84	0.07	0.07	0.86
NOV 1976			DEC 1976			JAN 1977			FEB 1977		
0.94	0.01	0.05	0.93	0.01	0.06	0.92	0.02	0.06	0.94	0.01	0.05
0.29	0.56	0.14	0.26	0.60	0.14	0.23	0.61	0.16	0.25	0.61	0.14
0.06	0.06	0.88	0.06	0.06	0.88	0.05	0.06	0.90	0.06	0.07	0.88
MAR 1977			APR 1977			MAY 1977			JUN 1977		
0.95	0.01	0.05	0.94	0.01	0.05	0.94	0.01	0.05	0.92	0.01	0.06
0.22	0.63	0.15	0.24	0.61	0.16	0.24	0.61	0.15	0.26	0.61	0.13
0.05	0.06	0.89	0.05	0.06	0.89	0.05	0.06	0.89	0.10	0.10	0.81
JUL 1977											
0.93	0.01	0.06									
0.31	0.56	0.13									
0.07	0.07	0.86									

Table 3

EXPECTED TRANSITION PROBABILITIES OF NONWHITE MEN AGED25-59

NOV 1975			DEC 1975			JAN 1976			FEB 1976		
0.97	0.01	0.01	0.97	0.02	0.01	0.96	0.02	0.01	0.96	0.02	0.01
0.16	0.67	0.17	0.17	0.67	0.16	0.13	0.67	0.21	0.14	0.73	0.13
0.07	0.06	0.87	0.07	0.07	0.86	0.06	0.05	0.89	0.08	0.09	0.83
MAR 1976			APR 1976			MAY 1976			JUN 1976		
0.98	0.01	0.01	0.98	0.01	0.01	0.98	0.01	0.01	0.97	0.02	0.01
0.15	0.67	0.17	0.19	0.65	0.16	0.20	0.63	0.16	0.15	0.69	0.15
0.06	0.08	0.85	0.07	0.07	0.87	0.07	0.07	0.86	0.07	0.07	0.86
JUL 1976			AUG 1976			SEP 1976			OCT 1976		
0.97	0.02	0.01	0.97	0.02	0.01	0.98	0.01	0.01	0.98	0.01	0.01
0.16	0.63	0.21	0.17	0.64	0.19	0.19	0.70	0.11	0.19	0.69	0.12
0.06	0.09	0.85	0.06	0.06	0.88	0.09	0.08	0.83	0.06	0.06	0.87
NOV 1976			DEC 1976			JAN 1977			FEB 1977		
0.97	0.01	0.01	0.97	0.02	0.01	0.96	0.02	0.01	0.98	0.02	0.01
0.15	0.67	0.17	0.17	0.67	0.16	0.13	0.67	0.21	0.14	0.73	0.13
0.07	0.06	0.87	0.07	0.07	0.86	0.06	0.05	0.89	0.08	0.09	0.83
MAR 1977			APR 1977			MAY 1977			JUN 1977		
0.98	0.01	0.01	0.98	0.01	0.01	0.98	0.01	0.01	0.97	0.02	0.01
0.15	0.67	0.17	0.19	0.65	0.16	0.20	0.63	0.16	0.16	0.69	0.15
0.06	0.08	0.85	0.07	0.07	0.87	0.07	0.07	0.86	0.07	0.07	0.86
JUL 1977											
0.97	0.02	0.01									
0.16	0.63	0.21									
0.06	0.09	0.85									

Table 4

EXPECTED TRANSITION PROBABILITIES OF WHITE MEN AGED 16-19

NOV 1975

0.87	0.04	0.09
0.20	0.59	0.22
0.09	0.07	0.84

DEC 1975

0.88	0.04	0.09
0.21	0.56	0.23
0.13	0.09	0.78

JAN 1976

0.86	0.05	0.09
0.17	0.55	0.28
0.09	0.06	0.85

FEB 1976

0.90	0.03	0.07
0.18	0.62	0.20
0.09	0.07	0.84

MAR 1976

0.90	0.03	0.07
0.19	0.58	0.23
0.10	0.07	0.83

APR 1976

0.90	0.03	0.07
0.22	0.52	0.26
0.12	0.07	0.80

MAY 1976

0.89	0.05	0.08
0.20	0.52	0.27
0.13	0.07	0.80

JUN 1976

0.89	0.05	0.06
0.26	0.57	0.17
0.33	0.21	0.47

JUL 1976

0.21	0.04	0.05
0.29	0.54	0.16
0.29	0.17	0.55

AUG 1976

0.88	0.03	0.09
0.24	0.52	0.24
0.18	0.10	0.72

SEP 1976

0.72	0.04	0.24
0.22	0.52	0.26
0.13	0.09	0.78

OCT 1976

0.86	0.03	0.10
0.23	0.56	0.21
0.13	0.08	0.79

NOV 1976

0.87	0.04	0.08
0.21	0.58	0.22
0.10	0.07	0.83

DEC 1976

0.88	0.04	0.09
0.21	0.56	0.23
0.13	0.09	0.78

JAN 1977

0.86	0.05	0.09
0.17	0.55	0.28
0.09	0.06	0.85

FEB 1977

0.90	0.03	0.07
0.18	0.62	0.20
0.09	0.07	0.84

MAR 1977

0.90	0.03	0.07
0.19	0.58	0.23
0.10	0.07	0.83

APR 1977

0.90	0.03	0.07
0.22	0.52	0.26
0.12	0.07	0.80

MAY 1977

0.89	0.03	0.08
0.20	0.52	0.27
0.13	0.07	0.80

JUN 1977

0.89	0.05	0.06
0.26	0.57	0.17
0.33	0.21	0.47

JUL 1977

0.21	0.04	0.05
0.29	0.54	0.16
0.29	0.17	0.55

Simulation of Expected Weeks of Work for a Sample of Job-Creation Program Enrollees

We have records on a stratified cluster sample of 2,000 Title X workers. Each is characterized by age (at time of program entry), race and sex, employment status in the month prior to program entry, and the months of program entry and exit.

The person's age/race/sex tell us which of the 16 transition matrix strings to consult. The entry and exit months tell us where to enter and from which point to leave the string. The pre-enrollment status tells us through which row (or "window") of the entry month matrix we are to enter (row 1 = "employed", row 2 = "unemployed", row 3 = "not in the labor force"). We then "walk" the person through the subsystem of transition probabilities so selected, compute the cumulative number of expected months spent in the "employed" state, and multiply that number by 4.3 (average weeks in a month).

For each cohort, it is possible to compute a table of expected weeks of employment for all possible entry/exit month pairs, for each of the three possible pre-enrollment statuses. For each Title X worker, we can then consult the appropriate pre-computed table, as an alternative to actually computing the month-by-month expectations for each of the 2,000 workers in our sample. The Fortran extract we wrote to simulate these expectations is reproduced as Figure 2,[16/] followed by a sample of output tables for four of the 16 cohorts (tables 5-8) [for a complete set of results, refer to fn. 15]. Referring to Table 5 by way of example, we estimate that a previously

---

16/ In the program extract, TP(I,J,K) is a transition matrix for month K, whose rows (I = 1,2,3) represent employment status at the beginning of the month, and whose columns (J = 1,2,3) represent the status at the end of the month.

Figure 2

Fortran Simulation Algorithm

```
C
C       HERE WE GENERATE THE TABLES OF EXPECTED WEEKS WORKED BY
C       ENTRY MONTHS (DOWN) AND EXIT MONTHS (ACROSS)
C
        DO 4141 II=1,3
        DO 123 JJ=2,22
        DO 123 KK=JJ,22
C
C       COMPUTE ESTIMATED WEEKS OF EMPLOYMENT
C
C       II = PRE-PROGRAM EMPLOYYMENT STATUS (1,2,3)
C       JJ = ENTRY MONTH (NOV 1975=2)
C       KK = EXIT MONTH (NOV 1975=2)
C
        JJ1 = JJ+1
        KK1 = KK-1
        RANGE=1+(KK-JJ)
        FACTOR=RANGE
        IF(RANGE.GT.4)RANGE=4
        DO 102 I=1,3
        DO 102 J=1,3
        W(I,J)=TP(I,J,JJ1)
        Q(I,J)=0.0
        A(I)=0.0
102     CONTINUE
        DO 1002 I=1,4
        WEEKS(I)=0.0
1002    CONTINUE
        TWKS=0.0
        WKS=0.0
C
C       END OF INITIALIZATIONS FOR EWKS COMPUTATIONS
C
        GOTO (110,111,112,113),RANGE
C
C       MODULE FOR 1 MONTH IN PROGRAM
C
110     WEEKS(RANGE)=TP(II,1,JJ)
        GOTO 100
C
C       MODULE FOR 2 MONTHS IN PROGRAM
C
111     DO 1111 I=1,3
        TWKS = TWKS + TP(II,I,JJ)*TP(I,1,KK)
1111    CONTINUE
        WEEKS(RANGE)=TWKS
        GOTO 100
C
```

Figure 2
(cont.)

```
C       MODULE FOR 3 MONTHS IN PROGRAM
C
112     DO 1112 I=1,3
        DO 1112 J=1,3
        A(J)=A(J)+TP(II,J,JJ)*TP(J,I,JJ1)
1112    CONTINUE
        DO 1113 I=1,3
        TWKS=TWKS+A(I)*TP(I,1,KK)
1113    CONTINUE
        WEEKS(RANGE)=TWKS
        GOTO 100
C
C       MODULE FOR 4 MONTHS OR MORE IN PROGRAM
C
113     DO 1023 LL=JJ1,KK1
        LLL=LL+1
        DO 1022 I=1,3
        DO 1022 J=1,3
        DO 1022 K=1,3
        Q(I,J)=Q(I,J)+W(I,K)*TP(K,J,LLL)
1022    CONTINUE
        DO 1023 I=1,3
        DO 1023 J=1,3
        W(I,J)=Q(I,J)
        Q(I,J)=0.0
1023    CONTINUE
        DO 1024 I=1,3
        DO 1024 J=1,3
        A(J)=A(J)+TP(II,J,JJ)*W(J,I)
1024    CONTINUE
        DO 1025 I=1,3
        TWKS=TWKS+A(I)*TP(I,1,KK)
1025    CONTINUE
        WEEKS(RANGE)=TWKS
C
C       TOTAL THE EMPLOYMENT PROBABILITIES,
CC      AND MULTIPLY BY THE NO. OF MONTHS
C       IN THE PROGRAM AT 4.3 WEEKS PER MONTH
C
100     DO 1026 I=1,4
        WKS=WKS+WEEKS(I)
1026    CONTINUE
        WKS=WKS*4.3*FACTOR
        EWKS(JJ,KK)=WKS
123     CONTINUE
C
        IF(II.EQ.1)WRITE(6,9011)
        IF(II.EQ.2)WRITE(6,9012)
        IF(II.EQ.3)WRITE(6,9013)
        DO 4142 I=2,22
        DO 4142 J=2,22
        IF(J.LT.I)EWKS(I,J)=0.0
4142    CONTINUE
        DO 4141 I=2,22
        WRITE(6,9009)(EWKS(I,J),J=2,22)
CC        WRITE(10,9008)(EWKS(I,J),J=2,22)
9008    FORMAT(13F6.2/9F6.2)
4141    CONTINUE
```

Table 5

Nonwhite Women Aged 20-24

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY EMPLOYED WORKERS

4.15	8.61	11.75	15.14	20.22	24.07	28.13	31.85	35.95	39.76	[illegible]	[illegible]	51.84	56.05	58.38	64.56	68.75	72.20	76.07	[illegible]	[illegible]
	4.15	7.25	[illegible]	[illegible]	20.05	24.03	27.37	31.95	35.78	[illegible]	44.15	47.85	52.44	54.94	60.52	64.70	69.18	72.25	[illegible]	[illegible]
		4.06	7.91	11.90	15.73	19.68	23.43	27.42	31.19	34.33	39.37	43.02	47.11	50.05	55.39	59.43	62.90	[illegible]	[illegible]	74.43
			4.19	8.15	12.11	16.18	20.06	24.15	28.04	31.31	36.40	40.19	44.36	47.46	[illegible]	57.06	60.64	64.73	68.22	72.43
				4.19	8.10	12.15	16.06	20.17	24.09	27.89	[illegible]	36.25	40.44	43.60	48.91	53.11	55.73	60.52	[illegible]	68.53
					4.16	8.05	11.97	16.02	19.93	23.73	28.17	31.95	36.12	39.35	44.50	48.05	52.27	56.34	59.57	64.07
						4.16	7.96	12.01	15.94	19.78	[illegible]	27.98	32.11	35.41	40.45	44.60	48.26	52.32	55.34	60.07
							4.12	7.94	11.86	15.59	19.96	23.79	27.87	31.22	36.11	40.21	43.86	47.90	51.47	[illegible]
								4.14	7.93	[illegible]	[illegible]	19.89	23.95	27.41	[illegible]	[illegible]	[illegible]	44.05	[illegible]	[illegible]
									4.12	[illegible]	[illegible]	15.84	19.87	23.33	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
										[illegible]	7.91	11.79	15.73	19.34	[illegible]	27.90	31.61	35.81	39.25	[illegible]
											4.16	7.99	12.04	15.73	20.22	24.32	28.14	[illegible]	[illegible]	[illegible]
												4.13	7.98	11.73	16.07	[illegible]	23.97	27.99	[illegible]	[illegible]
													4.15	7.85	12.10	[illegible]	20.05	24.03	27.37	[illegible]
														4.06	7.91	[illegible]	15.73	19.68	23.43	[illegible]
															[illegible]	8.15	12.13	16.18	20.06	24.15
																4.19	8.10	12.15	[illegible]	[illegible]
																	4.16	8.05	11.97	16.32
																		4.16	7.98	12.01
																			4.12	[illegible]
																				4.14

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY UNEMPLOYED WORKERS

0.15	0.57	0.55	0.71	1.25	1.46	2.07	2.42	2.45	3.05	3.55	3.70	3.83	3.99	4.81	3.93	4.29	4.37	5.61	6.05	5.72
	0.15	0.65	0.75	1.03	1.23	1.80	2.15	2.22	2.73	3.27	3.44	3.59	3.76	4.54	3.75	4.11	4.19	5.39	5.34	[illegible]
		0.20	0.57	0.90	1.12	1.59	2.09	2.26	2.81	3.29	3.54	3.73	3.97	4.72	4.12	4.49	4.49	5.76	[illegible]	6.13
			0.10	0.42	0.60	1.01	1.31	1.39	1.65	2.25	2.40	2.55	2.67	3.36	2.64	2.94	3.00	4.05	[illegible]	4.15
				0.12	0.41	0.79	1.09	1.21	1.65	[illegible]	2.22	2.37	2.53	3.19	2.57	2.85	2.90	3.96	4.35	[illegible]
					0.14	0.57	0.88	1.05	1.49	[illegible]	2.10	2.29	2.48	3.12	2.59	2.91	3.06	4.02	4.41	[illegible]
						0.17	0.65	0.89	1.31	1.72	1.99	2.22	2.46	3.05	[illegible]	[illegible]	3.15	[illegible]	4.56	[illegible]
							0.17	0.59	0.97	[illegible]	1.64	1.89	2.13	2.71	2.40	2.73	2.55	3.77	4.19	[illegible]
								0.13	0.57	[illegible]	[illegible]	1.37	1.58	2.10	1.79	2.07	2.20	3.01	[illegible]	3.27
									0.17	[illegible]	[illegible]	1.25	1.52	[illegible]	1.86	2.18	2.38	3.16	3.55	3.50
										0.20	0.71	1.03	1.33	1.83	1.76	2.10	2.34	3.09	[illegible]	3.51
											0.18	0.64	0.93	1.39	1.36	1.68	1.90	[illegible]	2.96	[illegible]
												0.17	0.60	1.01	1.05	1.35	1.56	2.19	2.55	2.60
													0.15	0.65	0.75	1.03	1.23	[illegible]	2.15	[illegible]
														0.20	0.59	0.90	1.12	[illegible]	[illegible]	[illegible]
															0.10	0.47	0.60	1.01	1.31	1.79
																0.12	0.41	0.79	1.09	1.21
																	0.14	0.57	[illegible]	[illegible]
																		0.17	0.65	[illegible]
																			0.17	[illegible]
																				0.13

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY NOT IN THE LABOR FORCE

0.11	0.45	0.75	0.82	1.00	0.70	1.51	2.01	2.26	2.62	2.82	[illegible]	3.02	[illegible]	3.73	3.29	3.41	2.11	4.11	5.02	5.28
	0.12	0.52	0.66	0.85	0.66	1.38	1.85	2.12	2.47	[illegible]	[illegible]	2.95	3.13	3.67	3.29	3.43	2.23	4.14	[illegible]	[illegible]
		0.15	0.44	0.71	0.61	1.25	1.71	2.00	2.35	[illegible]	2.68	2.93	3.16	3.67	3.36	3.53	2.44	4.26	[illegible]	[illegible]
			0.10	0.39	0.33	0.84	1.22	1.47	1.79	[illegible]	2.72	2.75	[illegible]	2.91	2.59	2.71	1.65	3.35	[illegible]	[illegible]
				0.10	0.21	0.61	0.95	1.19	1.50	1.71	[illegible]	1.95	2.15	2.60	2.31	2.44	1.45	3.05	[illegible]	[illegible]
					0.00	0.23	0.46	0.61	0.82	[illegible]	1.08	1.07	1.15	1.52	1.14	1.17	0.22	[illegible]	2.29	[illegible]
						0.11	0.50	0.75	1.05	[illegible]	[illegible]	1.63	1.87	2.24	2.06	2.21	1.40	[illegible]	[illegible]	[illegible]
							0.15	0.58	0.90	[illegible]	1.46	1.63	1.86	2.50	2.19	[illegible]	1.76	3.07	[illegible]	[illegible]
								0.15	0.60	[illegible]	1.18	1.37	1.61	2.02	1.97	[illegible]	1.62	[illegible]	[illegible]	[illegible]
									0.16	[illegible]	0.91	1.12	1.38	1.78	1.78	[illegible]	1.50	2.65	[illegible]	[illegible]
										[illegible]	0.59	0.82	1.08	1.45	1.49	1.70	[illegible]	[illegible]	[illegible]	[illegible]
											0.15	0.54	0.80	1.15	1.22	1.44	1.11	[illegible]	[illegible]	[illegible]
												0.11	0.47	0.80	0.90	1.10	0.82	[illegible]	2.15	[illegible]
													0.12	0.52	0.66	0.85	0.66	[illegible]	[illegible]	[illegible]
														0.15	0.44	0.71	0.61	[illegible]	1.71	[illegible]
															0.10	0.57	[illegible]	[illegible]	[illegible]	[illegible]
																0.10	0.21	[illegible]	[illegible]	[illegible]
																	0.03	0.23	[illegible]	[illegible]
																		0.11	[illegible]	[illegible]
																			0.15	[illegible]
																				[illegible]

Table 6

White Women Aged 20-24

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY EMPLOYED WORKERS

4.06	7.67	11.25	15.35	[illegible]	[illegible]	26.72	[illegible]	34.08	37.55	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	65.67	69.19	72.54	75.55	79.52
	4.02	7.44	11.42	15.31	19.05	22.72	26.23	30.05	33.87	35.23	41.78	45.85	49.13	[illegible]	[illegible]	61.25	64.72	[illegible]	71.21	75.13
		3.96	7.52	11.34	15.06	18.70	22.23	26.00	29.76	32.74	37.36	41.55	44.82	47.73	52.46	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
			4.05	7.71	11.51	15.24	18.66	22.63	26.52	29.11	34.10	38.46	41.94	44.95	47.83	53.45	57.55	[illegible]	[illegible]	[illegible]
				4.07	7.72	11.50	15.17	19.01	22.85	[illegible]	30.75	34.81	[illegible]	41.46	45.24	50.33	54.07	[illegible]	[illegible]	[illegible]
					4.06	7.64	11.33	15.15	18.57	21.91	26.80	30.82	34.29	37.52	42.21	[illegible]	49.55	53.45	56.55	60.53
						4.03	7.51	11.30	15.07	18.1	22.85	26.81	30.31	[illegible]	[illegible]	[illegible]	45.05	49.34	[illegible]	56.48
							3.98	7.45	11.19	[illegible]	18.93	22.73	26.22	29.50	33.95	37.93	41.54	[illegible]	[illegible]	52.15
								3.99	7.48	[illegible]	15.09	18.97	22.52	25.85	30.24	34.21	37.64	41.36	[illegible]	[illegible]
									4.00	7.22	11.55	15.22	18.82	22.24	26.54	30.50	34.16	37.71	[illegible]	[illegible]
										[illegible]	7.30	11.01	14.52	17.85	21.94	25.73	29.79	32.72	[illegible]	[illegible]
											4.03	7.67	11.37	14.93	[illegible]	23.04	26.77	30.39	[illegible]	37.68
												4.06	7.63	11.28	[illegible]	19.51	23.10	26.77	[illegible]	[illegible]
													4.02	7.44	[illegible]	[illegible]	19.06	22.72	[illegible]	[illegible]
														3.95	7.52	11.34	[illegible]	18.70	[illegible]	[illegible]
															4.05	7.71	11.51	15.24	[illegible]	[illegible]
																4.07	7.72	11.50	[illegible]	[illegible]
																	4.05	7.64	11.33	[illegible]
																		4.03	7.51	11.30
																			3.98	7.45
																				3.99

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY UNEMPLOYED WORKERS

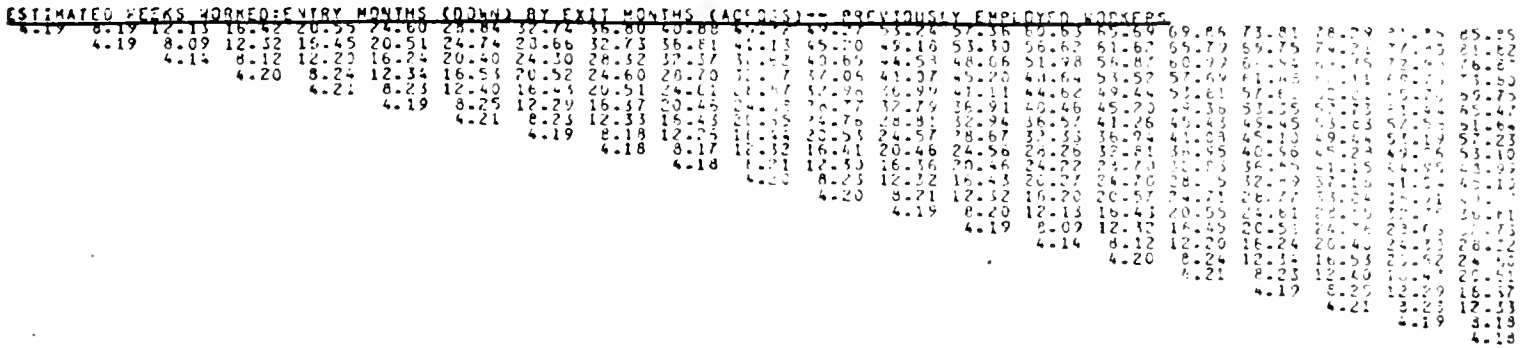
1.23	3.65	5.21	7.29	8.64	10.65	12.43	14.67	17.51	19.72	20.17	23.39	24.97	25.62	26.04	29.13	29.39	31.97	33.73	36.66	40.95
	1.12	3.33	5.24	6.60	8.51	10.21	12.34	15.04	16.23	17.69	20.71	22.27	22.86	23.29	26.21	26.41	28.97	30.63	33.49	37.61
		1.00	3.29	4.64	6.39	8.00	10.00	12.48	13.63	15.03	17.84	19.26	19.59	20.31	23.04	23.19	25.56	27.18	29.99	[illegible]
			1.09	3.26	5.04	6.73	8.72	11.18	12.50	14.02	15.72	18.34	19.14	17.73	22.47	22.82	[illegible]	[illegible]	[illegible]	[illegible]
				0.93	3.10	4.66	6.49	8.73	9.99	11.45	[illegible]	15.40	16.15	16.69	19.22	[illegible]	21.72	[illegible]	[illegible]	[illegible]
					1.02	3.23	5.05	7.20	8.60	10.14	12.60	14.14	15.07	15.79	[illegible]	16.75	[illegible]	[illegible]	[illegible]	28.79
						1.03	3.39	5.45	6.94	8.53	10.90	12.49	13.52	14.34	15.77	17.35	[illegible]	21.23	[illegible]	[illegible]
							1.11	3.76	5.41	7.03	9.42	11.12	12.31	13.30	[illegible]	16.49	[illegible]	20.41	[illegible]	[illegible]
								1.32	3.89	5.71	8.07	9.97	11.43	12.65	15.17	15.06	18.53	20.37	[illegible]	26.22
									1.20	3.65	5.85	7.65	9.10	10.35	12.67	13.72	[illegible]	[illegible]	[illegible]	[illegible]
										1.20	3.88	5.74	7.24	8.62	10.66	12.00	14.11	15.77	19.21	[illegible]
											1.31	3.99	5.70	7.23	9.45	10.80	12.94	14.77	[illegible]	[illegible]
												1.26	3.69	5.26	7.35	8.74	10.76	12.55	[illegible]	[illegible]
													1.12	3.33	5.24	6.60	8.51	10.21	[illegible]	[illegible]
														1.00	3.29	4.64	6.39	8.00	10.00	12.48
															1.09	3.26	5.04	6.73	8.72	11.18
																0.93	3.10	4.66	[illegible]	[illegible]
																	1.02	3.23	[illegible]	7.20
																		1.03	3.39	[illegible]
																			1.11	3.76
																				1.32

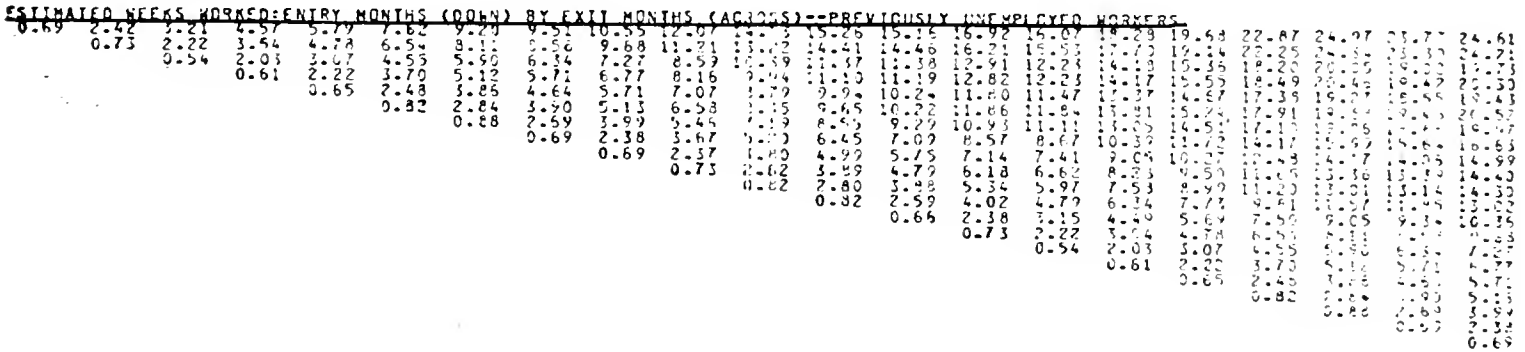
ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY NOT IN THE LABOR FORCE

0.24	1.04	1.40	2.05	2.43	2.87	3.33	5.23	5.28	5.39	6.41	[illegible]	6.90	7.25	7.50	8.18	8.25	8.61	9.18	13.07	12.31
	0.26	0.97	1.52	2.02	2.49	3.01	4.76	4.84	5.02	6.00	6.65	6.60	6.97	6.79	7.95	8.05	8.45	9.23	[illegible]	12.10
		0.21	0.96	1.37	1.80	2.27	3.76	3.90	4.08	5.00	5.57	5.51	5.87	5.70	6.75	6.46	7.19	7.77	11.29	[illegible]
			0.25	1.00	1.45	1.99	3.34	3.61	3.88	4.77	5.41	5.46	5.86	5.77	6.84	7.00	7.40	7.76	11.34	[illegible]
				0.23	0.94	1.42	2.58	2.88	3.17	4.01	4.60	4.68	5.07	5.02	6.01	6.19	6.55	7.09	10.31	[illegible]
					0.22	0.92	1.90	2.26	2.59	3.37	2.95	4.07	4.46	4.45	5.38	5.57	5.93	6.46	[illegible]	[illegible]
						0.22	1.20	1.71	2.10	2.94	[illegible]	3.61	4.02	4.07	4.96	5.14	5.56	6.08	[illegible]	[illegible]
							0.41	1.54	2.17	3.02	[illegible]	4.73	4.91	5.18	6.27	6.07	[illegible]	[illegible]	10.71	[illegible]
								0.32	1.24	1.98	2.67	3.07	3.59	3.43	4.74	5.10	5.00	6.21	8.73	[illegible]
									0.28	1.25	[illegible]	2.31	2.81	3.06	3.89	4.25	4.70	5.27	7.61	7.58
										0.34	1.38	1.91	2.46	[illegible]	[illegible]	4.10	4.66	5.25	7.45	[illegible]
											0.32	1.22	1.79	2.18	2.95	3.38	3.90	4.49	6.54	6.64
												0.26	1.07	1.45	2.12	2.52	2.98	3.51	5.36	5.43
													0.26	0.97	1.52	2.02	2.49	3.01	[illegible]	[illegible]
														0.21	0.96	1.37	1.00	2.27	3.76	3.92
															0.25	1.00	1.48	1.99	3.34	3.61
																0.23	0.94	1.42	[illegible]	[illegible]

Table 7

Nonwhite Men Aged 25-59





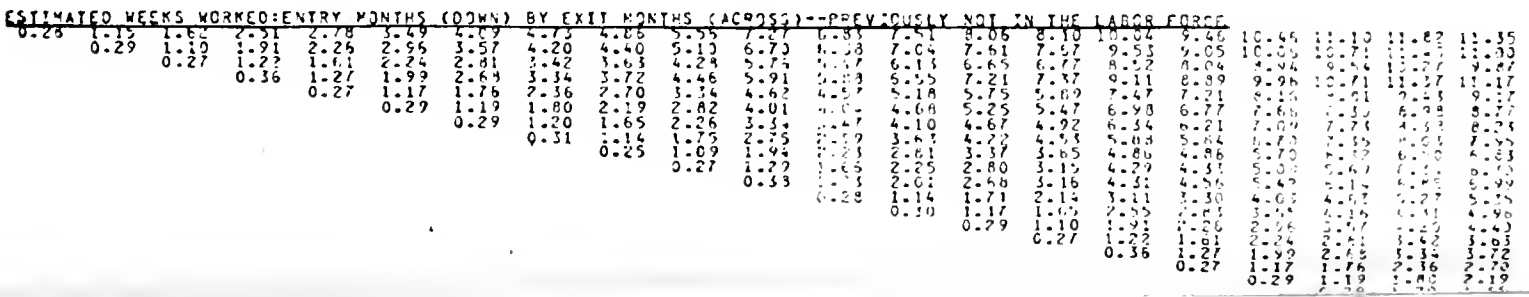


Table 8

White Men Aged 16-19

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY EMPLOYED WORKERS

3.75	6.75	9.55	13.67	17.23	20.84	23.94	[illegible]	[illegible]	33.76	30.78	39.70	[illegible]	47.54	49.17	54.75	58.47	62.53	64.97	[illegible]	74.85
	3.79	6.59	10.35	13.84	17.46	20.34	24.63	28.65	30.77	[illegible]	[illegible]	40.42	44.20	45.13	51.64	55.34	59.39	[illegible]	[illegible]	71.03
		3.67	6.75	10.17	13.71	16.87	20.76	24.67	26.83	24.87	32.81	[illegible]	40.03	42.00	47.23	[illegible]	54.03	57.35	[illegible]	65.97
			3.85	7.03	10.64	13.97	17.80	21.75	24.23	[illegible]	30.54	34.19	37.97	40.20	45.47	49.20	53.19	[illegible]	[illegible]	65.25
				3.86	7.11	10.51	14.27	18.17	20.85	20.03	27.25	30.87	34.62	36.97	42.11	45.43	[illegible]	[illegible]	[illegible]	[illegible]
					3.88	7.03	11.74	14.59	17.42	[illegible]	[illegible]	27.55	31.23	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
						3.62	7.09	10.83	13.78	[illegible]	20.24	23.79	27.46	29.57	34.77	[illegible]	42.31	[illegible]	[illegible]	[illegible]
							3.82	7.22	10.34	11.37	16.93	20.44	24.06	26.65	31.34	34.97	[illegible]	[illegible]	[illegible]	50.53
								3.90	7.00	8.67	13.75	17.53	20.97	23.76	28.36	32.05	35.92	[illegible]	[illegible]	47.57
									3.77	5.61	9.99	13.42	16.94	19.71	24.01	27.58	31.33	34.28	[illegible]	[illegible]
										3.11	5.70	8.55	11.60	13.92	17.43	20.47	23.79	[illegible]	[illegible]	[illegible]
											3.71	6.65	10.05	12.98	16.94	20.43	24.08	27.11	[illegible]	[illegible]
												3.75	6.77	9.24	13.70	17.21	20.66	[illegible]	[illegible]	[illegible]
													3.78	6.52	10.37	13.84	[illegible]	[illegible]	[illegible]	[illegible]
														3.69	6.75	10.17	17.71	[illegible]	[illegible]	[illegible]
															3.85	7.03	10.64	13.97	[illegible]	[illegible]
																3.86	7.11	10.51	[illegible]	[illegible]
																	3.88	[illegible]	[illegible]	14.59
																		3.62	[illegible]	[illegible]
																			3.82	7.22
																				3.90

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY UNEMPLOYED WORKERS

0.86	2.78	3.67	5.14	6.60	8.62	9.69	13.74	15.97	15.20	14.06	17.17	17.53	19.45	18.33	20.55	22.45	25.85	26.30	[illegible]	37.30
	0.93	2.52	3.97	5.43	7.35	9.53	12.29	14.47	13.97	13.04	[illegible]	16.59	16.52	17.62	19.44	21.73	25.02	[illegible]	[illegible]	[illegible]
		0.71	2.33	3.61	5.26	6.37	9.52	11.86	11.37	10.69	[illegible]	13.55	15.36	14.43	16.33	18.04	21.13	[illegible]	[illegible]	32.20
			0.76	2.54	4.16	5.34	8.31	10.55	10.31	9.95	12.50	13.02	14.74	14.05	16.02	17.77	[illegible]	[illegible]	[illegible]	[illegible]
				0.81	2.87	4.07	6.80	8.73	9.98	8.00	11.25	11.89	13.63	13.15	15.07	16.80	19.72	[illegible]	[illegible]	[illegible]
					0.95	2.90	5.59	7.37	7.99	7.91	10.33	11.23	13.05	12.85	14.84	16.60	19.44	[illegible]	26.55	29.49
						0.88	3.51	5.39	6.11	6.37	[illegible]	9.41	11.14	11.05	12.67	14.54	17.22	[illegible]	24.55	[illegible]
							1.13	3.91	5.15	5.77	[illegible]	9.38	11.20	11.51	13.60	15.45	[illegible]	[illegible]	24.51	[illegible]
								1.27	3.61	4.54	5.90	8.28	10.15	10.74	12.83	14.80	17.49	[illegible]	[illegible]	[illegible]
									1.04	2.74	[illegible]	5.89	7.55	8.12	9.94	[illegible]	14.07	[illegible]	[illegible]	[illegible]
										0.94	2.94	4.17	5.75	6.35	8.01	9.59	11.85	12.91	[illegible]	[illegible]
											1.00	2.92	4.50	5.35	7.02	8.64	10.86	[illegible]	[illegible]	[illegible]
												0.89	2.84	3.76	5.27	6.76	9.60	[illegible]	[illegible]	[illegible]
													0.90	2.52	3.97	5.43	7.36	8.53	[illegible]	[illegible]
														0.71	2.35	3.61	5.28	6.37	[illegible]	[illegible]
															0.76	2.54	4.16	5.34	[illegible]	[illegible]
																0.81	2.87	4.07	[illegible]	[illegible]
																	0.95	2.90	[illegible]	[illegible]
																		0.88	[illegible]	[illegible]
																			1.13	3.91
																				1.27

ESTIMATED WEEKS WORKED:ENTRY MONTHS (DOWN) BY EXIT MONTHS (ACROSS)--PREVIOUSLY NOT IN THE LABOR FORCE

0.40	1.77	[illegible]	2.76	[illegible]	5.27	8.20	[illegible]	13.58	10.86	9.01	[illegible]	10.03	12.36	10.65	11.79	12.95	[illegible]	[illegible]	[illegible]	[illegible]
	0.56	1.67	2.61	3.55	5.03	6.07	11.83	12.68	10.72	9.19	[illegible]	10.61	13.08	11.71	13.03	14.32	[illegible]	[illegible]	[illegible]	[illegible]
		0.35	1.43	2.27	3.42	4.33	9.57	10.30	8.54	7.22	[illegible]	8.25	10.34	8.95	10.00	[illegible]	13.69	[illegible]	[illegible]	[illegible]
			0.37	1.50	2.60	3.51	7.98	8.87	7.54	6.59	7.73	7.52	9.60	8.40	9.44	10.52	[illegible]	[illegible]	[illegible]	[illegible]
				0.41	1.77	2.69	6.44	7.47	6.56	5.79	7.22	7.33	8.92	7.91	8.95	10.03	[illegible]	[illegible]	[illegible]	[illegible]
					0.54	1.98	5.04	6.28	5.90	5.40	6.78	7.01	8.86	8.14	9.30	10.45	12.78	13.85	[illegible]	[illegible]
						0.56	3.39	4.76	4.78	4.57	5.91	6.26	8.01	7.49	8.64	9.79	12.02	[illegible]	[illegible]	[illegible]
							1.40	4.22	5.43	5.85	8.54	9.64	11.74	12.19	14.32	16.19	[illegible]	[illegible]	[illegible]	[illegible]
								1.24	3.37	4.05	[illegible]	7.30	9.24	9.69	11.55	13.23	15.68	[illegible]	[illegible]	[illegible]
									0.79	2.12	[illegible]	4.34	5.92	6.11	7.42	8.69	10.73	[illegible]	[illegible]	[illegible]
										0.55	[illegible]	2.70	4.01	4.17	5.20	6.25	9.04	[illegible]	[illegible]	[illegible]
											0.54	1.77	2.97	3.28	4.26	5.26	6.52	7.95	[illegible]	[illegible]
												0.43	1.81	2.20	3.04	3.92	[illegible]	[illegible]	[illegible]	[illegible]
													0.56	1.67	2.61	3.53	5.03	[illegible]	[illegible]	[illegible]
														0.36	1.43	2.22	3.42	4.33	[illegible]	[illegible]
															0.39	1.50	2.60	3.51	[illegible]	[illegible]
																[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

unemployed nonwhite woman aged 20-24 who entered this particular job-creation program in its third month and left in month ten would, had she never encountered Title X, have experienced 2.81 weeks of employment somewhere in the economy, anyway.

What does this methodology assume? It assumes that month-to-month and season-to-season changes (not levels) in the employment status-demography-labor-market nexus during the Title X period are likely to have about the same shape as those in the Urban Institute sample period 1967-73. And it assumes that Title X is generally too small a program to "feed back" and actually affect the national levels of the instrumental variables (unemployment and vacancy rates). These seem to us to be reasonable assumptions.

The assumption that last month's status determines this month's expectations -- within a cohort -- is, however, certainly not appropriate for all enrollees in a program like Title X. How, for example, does one treat long term or structurally unemployed persons when we are trying to estimate the expected number of weeks of employment in the absence of Title X? We should think that a person who has been unemployed for 26 weeks will be less likely to have the same employment experience (over the Title X period) as a person who was unemployed for only 5 weeks prior to Title X. However, the Smith-Vanski-Holt model presented previously has as its starting point the labor force status of the person in the month prior to entrance into the model. None of the movement in the model depends on the past history of the individual (before month t-1). Duration of previous employment status is not considered.[17/]

17/ For the US population sampled randomly, the assumption that "history doesn't count" may not matter; the long term unemployed are a small enough proportion of the total labor force for the Urban Institute model to have unbiased coefficients. For our Title X sample, on the other hand, the group of workers who were previously unemployed for 26 weeks or longer are in fact the modal group.

If all Title X participants were entered into the model without regard to their employment histories, we believe we would seriously overestimate the number of weeks a person would have been employed in the absence of the program. We have therefore adjusted "entrance" into the model in the following manner:

(1) Some persons were employed at the time of their entry into Title X. Others were unemployed or had not been in the labor force for a period of 26 weeks or less prior to their entering Title X. Such persons are entered into the model according to their status in the month prior to Title X hiring.

(2) All persons who were unemployed or out of the labor force for more than 26 weeks are not entered into the model at all. The result of this qualification is to count all of the Title X employment of these persons as an addition to their work experience.

This decision is, of course, arbitrary. The 26 week cutoff point is a simplifying assumption, which seems consistent with BLS definitions of "long term unemployment". It is a conservative decision rule, since 81.7% of the unemployed during 1976 had been in that state for 26 weeks or less.[18/] And it fits well with the intent of the Title X program. Since Title X is a countercyclical program and targeted at those who were unemployed due to the economic slump dating to mid-1974, the degree to which the structural or long term unemployed were aided by the program can be viewed as a desirable side benefit. The data collected by Abt for EDA show, somewhat surprisingly, that approximately one-third of those working on Title X projects had been unemployed for over 26 weeks. In fact, over 15 percent of the participants had

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18/ Bureau of Labor Statistics, Employment and Earnings, Jan. 1977, p. 149.

been unemployed for more than 52 weeks, clearly in the "structural" category. These figures indicate that the program, even though not required to do so by legislation or regulations, employed many who were most in need of work.

Finally, we should note that the Smith et al. model has been estimated with national CPS and Conference Board data. Yet we are using it to simulate the behavior of "control groups" of workers from EDA Title X target areas. These were areas of high unemployment and low growth, where the opportunities for non-subsidized employment are presumably fewer than in the economy as a whole. Thus, our estimates of alternative work opportunity in the absence of Title X must be biased upward. This, then, confers a further conservative direction to our estimates of the net job-creation impact of Title X.

Comparing the Net and Gross Private Employment Benefits from the Job Opportunities Program

The 2,000 Title X workers in our sample were employed in the program for an average of 35.4 weeks. In the absence of the Program, we estimate that they would have found an average of 10.4 weeks of work anyway (using the methodology illustrated above). This particular federal job-creation program appears, therefore, to have created an average of 25 person-weeks of employment for each enrollee during the 21 months of the program's existence. In other words, only about 71% of the "new" work can actually be attributed to Title X itself. Applying this fraction to the total number of direct full-time-equivalent jobs actually filled in state and local development projects, which we estimate at 76,900 on the assumptions of zero budget substitution and displacement, our conclusion is that the Economic Development Administration's expenditure of $758 million directly produced

about 54,600 new jobs (not counting multiplier effects). To the extent that Title X did suffer from the leakages associated with budget substitution or displacement, the net direct benefits will have been smaller.[19/]

In this paper, we have reported on our experiments with a new methodology for program evaluation. In the absence of a closely-matched control group of non-Title X persons, we resorted to the construction of what amounts to a simulated control group instead. We were able to use the Smith-Holt-Vanski monthly national labor market transition model to generate a set of expected month-to-month careers for sixteen age/race/sex cohorts. The program-generated work experiences of a sample of actual Job Opportunities enrollees were then compared systematically with the simulated careers for the corresponding cohorts, in order to estimate how much work each actual enrollee might have been expected to have had, given his or her age, race and sex, even in the absence of Title X. Had we tried to evaluate the direct job-creation impact of the program by counting the actual number of weeks of work funded by the federal government, we would have exaggerated the impact of Title X (and, it should be noted, its social cost as well) by about 30%.

This magnitude seems large enough to warrant serious official attention on the part of federal agencies like EDA, the Dept. of Labor, HUD, etc. Consideration should be given to the "in-house" development of detailed, standardized stock-flow models such as the one employed in this paper, which could be used routinely to simulate control group behavior in the process of conducting agency program evaluations. Such developments need not and

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19/ The income, earnings and employment multiplier effects of this EDA expenditure are estimated in Effects of Job Creation, Vol. 2, Chap. 5. There, an additional assumption about leakage from the flow of funds is introduced: the possibility that the federal government will fail to respend all of the extra tax income or welfare and unemployment insurance savings that are generated by the extra jobs created by the program.

certainly should not replace experiments with true control groups. But the realities of survey costs and time pressures point to the need for computer-assisted approximations. Of course, only with much further research and experimentation will it be possible to find out how closely the behavior of such simulated control groups corresponds to that of the "real thing", and what elements of model design are the most instrumental in narrowing the differences.

BASEMENT
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HD28.M414 no.982- 78
Van Breda, M. /Accounting in a golden
735239 D*BKS 00057511
3 9080 001 100 699

HD28.M414 no.983- 78
Scott Morton, /Some perspectives on co
735236 D*BKS 00057869
3 9080 001 105 029

HD28.M414 no.984- 78
Harrison, Benn/Simulated control group
734916 D*BKS 00055244
3 9080 001 073 110

HD28.M414 no.985- 78
Lorange, Peter/Corporate planning , an
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